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The Repeatability Problem in ESP-Personality Research

J. FRASER NICOL AND BETTY M. HUMPHREY

. . . how important it is that the observations and experiments of science should be repeated as often and by as many observers as possible, in order to ensure that we are dealing with what has validity for all human beings . . .

Karl Pearson, *The Grammar of Science*

In an earlier paper by the present writers an account was given of a research in which fairly strong evidence was found of the existence of relationships between certain aspects of human personality and the ESP scores of the experimental subjects (11).

When those results became known, we were enabled by the Parapsychology Foundation, New York, to seek a repetition of those results by further research. It is a pleasure, before describing the conduct and outcome of the new research, to express our sense of indebtedness to the Foundation and especially to its President, Mrs. Eileen J. Garrett, for the very practical encouragement they have extended to us.[1]

[1]The program of research was conducted under the general aegis of the American Society for Psychical Research. Dr. Gardner Murphy has devoted much time to the work in an advisory capacity, and we are indebted to him also for bringing his critical acumen to bear on some of the more difficult portions of this paper. Mrs. E. W. Allison has assisted at many points, and Mrs. Laura Abbott Dale has helped with a number of useful comments on the original draft of the paper.

We shall first note briefly the leading findings of the previous research, then give an account of the new experiment, proceeding thereafter to make comparisons between the first research and the second, and finally, to discuss the difficult but insistent problem of establishing repeatability in psychical experimentation.

The research whose results form a substantial portion of this paper was designed as a repetition of the preceding work, the ultimate purpose of these investigations being to reconnoiter the problem of repeatability and to probe the boundaries, as it were, at as many places as possible simultaneously.

Later, it will be shown that there were unavoidable differences between the two experiments, but in their purely technical aspects of ESP procedures and personality testing the second research was as nearly as possible identical with the first.

In Research I we used two ESP experimental techniques: 1) "Unknown" tests, or the standard ESP test, where the subject was informed of his success or failure only at the end of each run, and 2) "Known" tests in which the subject was shown the actual target card after each guess, therefore providing him with immediate knowledge of his success or failure on each trial. The authors, as experimenters, conducted all the tests. One sat at a table near the subject and recorded his calls. The other sat several yards away and handled the randomized cards behind a wooden screen. On the Unknown runs the experimenter simply held each card face down and slightly apart from the pack until the subject had made his call; then without seeing the face of the card, the experimenter laid each card aside in a separate pile. After all 25 cards in the pack had been called, the subject's calls and the card order were compared and the hits counted.

The Known tests were conducted in similar fashion except that after each call, the face of the card was shown to the subject at a small opening or "window" in the middle of the wooden screen.

Generally after his experimental session (but in a few cases prior to it) the subject filled out three standard questionnaires yielding scores on a variety of personality factors. Also immediately before the ESP test, each subject took a modified form of Cason's Test of Annoyance. (These questionnaires together with examples of the types of questions used in each were described in our first report, 11.)

The gross ESP score for the 36 subjects tested in Research I was slightly above mean chance expectation but was of no significance. The *raison d'être* of the research, however, was the relationship of individual subject's ESP scores with certain personality qualities, and herein we found that generally ESP scores varied with percipients' ratings on some of these personality factors. Thus, using

the standard product-moment correlation, we found persons who rated high on self-confidence (from the Guilford-Martin questionnaire) also scored high on ESP, and conversely, those whose self-confidence was low obtained low ESP scores. Another striking correlation was found to exist between ESP and the factor of emotional stability (from Cattell's personality inventory). Those were the most significant personality effects found, but there were a number of others of significance which are listed below in relation to the type of ESP test for which they were of importance. The descriptions apply to percipients whose ESP scores were high (opposite descriptions relate to low-scoring ESP subjects):

Unknown ESP Score	Known ESP Score	Total ESP Score
Thinking extravert		
Happy-go-lucky (?)		
	Not depressed	Not depressed
	Not cycloid	
	Not nervous	Not nervous
	Not easily annoyed	
Calm trustful		Calm trustful

However, these various correlations with ESP were not mutually independent, the personality factors themselves being intercorrelated, and it is probable that several of the factors in the table are of only slight interest as determinants of ESP ability.

Those personality variables were in a sense the *stable* elements which the subject brought into the experimental room with him. Though personality cannot be described as immutable (for time and environment will change the hardest rock), yet it can be said that the factors measured in these tests change little over quite considerable periods of time. The ESP research, however, also included several factors which undergo extensive fluctuations during the hour or two of an ESP session. Thus, subjects were invited to remark whether they experienced any special feeling at the moment of giving some of their ESP calls. They reported these feelings with the single word "Check," and the recording experimenter entered a check mark opposite such calls. The results of this aspect of the two researches have already been reported (9).

The Plan of Research II

The ESP Test

The ESP procedures followed in Research II were the same as those for Research I. Since the details were given rather fully in the report on Research I (11, pp. 136-144), only a brief summary will be presented here.

Sixteen packs of ESP cards were called by each subject. Unknown runs were alternated with Known runs, resulting in a total of eight

runs for each type of test. Odd-numbered subjects began their sessions with an Unknown run, followed by a Known run, and so on through the 16 packs. Even-numbered subjects followed the reverse procedure, beginning their sessions with Known runs. For seventeen of the subjects JFN handled the cards for the first eight packs, while BMH recorded the subjects' calls; at the midpoint in these sessions, the experimenters changed places. For eighteen of the subjects, the session started with BMH holding the cards and JFN recording for the subject; again after eight runs, places were exchanged and the session continued with JFN handling the cards and BMH recording the calls.

The cards were prepared as for the later sections of Research I, their order being randomized on the basis of the numbers in Kendall and Smith's tables of random numbers (10). A full account of the method appears on page 137 of the first report.

When a batch of 200 packs was prepared, the cards were transferred to JFN's study. Just prior to each experimental session or series of consecutive sessions, only the number of packs needed was taken to the experimental room on the campus. At no time were the prepared cards left unguarded by one or other of the experimenters. Only when the test was ready to begin were the boxes of cards removed from their container behind the screen. As before, each pack was used only once.

The Experimental Room

So far as possible, humanly speaking, every endeavor was made in Research II to reproduce the conditions of Research I. The most apparent difference was that, while Research I was conducted in the Parapsychology Laboratory of Duke University, Research II took place in a seminar room (No. 324G) of the University Library building. In the absence of an experiment properly controlled for this purpose, it is needless to guess whether the size, proportions, and furnishings of a room may create different effects on the subjects and hence on their ESP scores. For the record it may be mentioned that the new room was smaller than the one previously used, the measurements being approximately 12 x 14 feet as compared with 13 x 20 feet. The room used in Research I was BMH's sanctum in the laboratory, and contained the accumulations of twelve years' work and study. It had all the qualities of a "den": a desk with one or two odds and ends of personal things; books, files, plants, one or two rugs, a couch and a floor lamp; pictures relieved the bareness of the walls; draperies framed the double window.

By contrast, the walls of the new room were bare except for a blackboard at one end, and a frosted glass door. An inventory of the contents was as follows: a very large cumbersome table which

dominated the place like a car in a garage; a bench divided for four students, eight or ten hard chairs. There was nothing else.

We imported into the room an easy chair for the subjects' accommodation, a small card-table, a wooden screen, and an electric fan. (See Figure 1.)

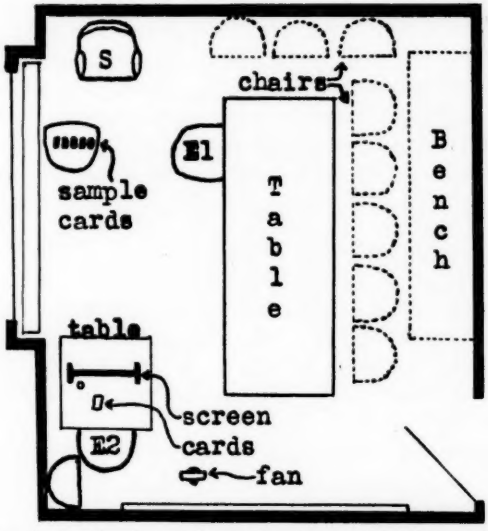

FIGURE 1. Rough sketch of experimental room used in Research II. Approximate dimensions: 12′ x 14′. "S" indicates position of subject, with chair in front of him upon which are displayed a sample of each of the five ESP symbols. "E1" and "E2" are the positions of the experimenters.

Because of the room's shape and its great unwieldy table, the position of the subject's chair was virtually predetermined. It was placed at the window corner, the array of four windows being slightly forward and on the subject's right. Approximately eight feet in front of the subject were the square card-table with the wooden screen on top and one of the experimenters with the cards seated at the other side. In Research I the subject had had his back to the window which faced north. In Research II the light streamed into his eyes from the west and it was often necessary to pull down the blind because of the unrelenting sun and the somber heat of a Carolina summer. (During the summer the temperature in Durham attained 103° in the shade; and was commonly well into the 90°'s during the actual card-calling.)

In every particular the information obtained from the researches was appraised by strictly quantitative methods; on the issue of environmental influences we have no means of applying such

methods. The precision, the informativeness, of experimental findings would no doubt be enhanced by the quantification of the environment in psychical research, but that stage still lies in the future.

The Percipients

The business of recruiting percipients may turn out to be one of the crucial factors on which some of the more unforeseen results described later in this paper ultimately depend, and it seems necessary to discuss some differences between the sources from which subjects were obtained in Research II as compared with Research I.

Of the 36 subjects tested in the first investigation, eleven could be described as either friends of ours or at least persons known to one or both of us. In the second research, all 35 subjects were strangers to us. In the first research 24 were women and 12, men; in the second research 18 were women and 17, men. Also in the first research all subjects were volunteers in the true sense of the word. As an example of how subjects were obtained, a university teacher read out our invitation to a class, gave the research friendly approval, and passed round a sheet on which students could write their names and the times at which they wished to come for the tests. Those who came did so because they were personally interested in trying the ESP tests.

The circumstances of the following year were different. Notwithstanding the same friendly interest of several university teachers as in the previous year, student subjects were not so readily available.[2] For one thing, Research II began toward the end of the spring semester when students were in the usual rush of finishing term papers, preparing for final exams, and participating in the end-of-the-year social events. After testing ten subjects during this period, the research was delayed several weeks until the start of summer school. Again it was not easy to secure subjects at this period, perhaps because of the rather tight schedule of summer school classes and the stifling heat. In this situation, one of our subjects, who was an enthusiast *par excellence,* offered to recruit subjects in considerable numbers. It soon emerged, however, that some of his recruits were by no means volunteers in the usual sense of the word; rather they came as a favor to their friend, and so far as we could judge their interest in ESP was mild or non-existent. Not surprisingly, some subjects were very dilatory in completing the three personality questionnaires, and we had to devise pertinacious methods to procure the return of the inventories.

Approximately 26 per cent of the subjects were secured through the efforts of the above-mentioned recruiter; 29 per cent responded

[2] To Dr. Wally Reichenberg-Hackett and to Dr. Hornell Hart we are especially indebted for their help in securing subjects.

to public notices requesting subjects; 31 per cent came as a result of classroom solicitation; and the remaining few were sent by friends. All were Duke students except three subjects who were young adults under thirty years of age.

The Personality Questionnaires

Before taking the ESP tests, each subject filled out a shortened form of Cason's Test of Annoyances (2). At the end of the session, each subject was asked to fill out at home three longer questionnaires and to return them by mail as soon as possible. (In Research II all subjects took these questionnaires after their ESP tests, while in Research I a few subjects filled out all or part of the inventories before taking the ESP tests.)

The three questionnaires used were:

1) J. P. Guilford's "An Inventory of Factors STDCR" (7), a questionnaire yielding scores on five aspects of personality: Social Introversion-Extraversion, Thinking Introversion-Extraversion, Depression, Cycloid Disposition, and Rhathymia (happy-go-lucky, carefree disposition).

2) The Guilford-Martin "Inventory of Factors GAMIN" (8), giving scores on five measures of temperament: General Activity Level, Ascendance-Submission, Masculinity-Femininity, Self-Confidence, and Nervousness.

3) R. B. Cattell's "Sixteen Personality Factor Questionnaire, Form A" (also known as "The 16 P.F. Test"), from which we selected, as in Research I, the nine most reliable scales: Cyclothymia, General Intelligence, Emotional Stability, Dominance, Surgency, Adventurous Cyclothymia, Bohemian Unconcernedness, Worrying Suspiciousness, and Nervous Tension (3).

These three inventories and Cason's Test of Annoyances were described rather fully, together with illustrations of the types of questions asked, in our earlier report (11, pp. 144-150) and these details will not be repeated here.

As was mentioned in the report of the first research, the Cattell questionnaire is still undergoing standardization, and since our first report new norms have been issued for scoring Form A of the Cattell test (4). Accordingly, we used the new norms in assessing the questionnaires of the subjects in Research II. And, because later in this paper we want to compare the two researches, it was also necessary to re-score the questionnaires from Research I by the new norms. Using the new scores for the subjects in Research I, we re-computed the correlations cited in the previous report and found that the revised personality scores affected the correlations little, except for a slight change upwards in the correlation of ESP

and emotional stability. In later tables in this paper where the correlations found in the first series are repeated, it will be noted that they do not agree precisely with those cited in our earlier report because of this re-scoring of the Cattell test; however, it will be noted also that no conclusions of that report are affected by the revisions.

General ESP Results

Table 1 displays the general ESP scores of Research II under several conditions: both for Unknown and Known test situations, and for the results obtained when the cards were under the control of one or other of the two experimenters. The total deviation of 62 is negative and is not significant ($\chi^2 = 1.72$; P = .18). The Known total as it stands is uninteresting; the Unknown total is the best of the results ($\chi^2 = 2.70$; P = .10) but is quite distant from the .05 significance level.

TABLE 1

General ESP Results, Research II

PAGE 1

Experimenter with Cards	Unknown Runs	Unknown Dev.	Known Runs	Known Dev.	Total Runs	Total Dev.
BMH	72	—17	72	—15	144	—32
JFN	68	—10	68	+11	136	+ 1
Total	140	—27	140	— 4	280	—31

PAGE 2

Experimenter with Cards	Unknown Runs	Unknown Dev.	Known Runs	Known Dev.	Total Runs	Total Dev.
BMH	68	—14	68	— 3	136	—17
JFN	72	—14	72	0	144	—14
Total	140	—28	140	— 3	280	—31

BOTH PAGES POOLED

Experimenter with Cards	Unknown Runs	Unknown Dev.	Known Runs	Known Dev.	Total Runs	Total Dev.
BMH	140	—31	140	—18	280	—49
JFN	140	—24	140	+11	280	—13
Total	280	—55	280	— 7	560	—62
	$\chi^2 = 2.70$ P = .10		$\chi^2 = .044$ P = .83		$\chi^2 = 1.72$ P = .18	

As in the previous series, although the Known runs yielded more hits than the Unknown, the difference is not significant. The subjects obtained more hits when JFN was holding the cards than when BMH was handling them; the same trend was noted in Research I, but in neither case was the difference significant.

There was no difference in the number of hits made on the two pages of each subject's data.

Personality Results of Research II

Of 35 subjects who underwent the psychic tests, 32 returned their personality records. The correlations of ESP scores with the personality characteristics are shown in Table 2. Factors in which we were most interested were those which were statistically significant in relation to total ESP scores in the previous research. Those factors were: Self-Confidence (Guilford-Martin), Freedom from Nervousness (two separate measures: Guilford-Martin and Cattell), Emotional Stability (Cattell), Absence of Worrying Suspiciousness (Cattell), and Low Irritability (Cason's Annoyance). Several other personality factors were significantly related to *either* the Known *or* Unknown tests, but not to the total ESP scores.

In view of the fact that for Research II the .05 significance level arises at a correlation coefficient of .349, none of the personality factors in which we were interested were significantly related to the total ESP scores of Research II. ***The new series does not confirm the old in the sense of providing a significant reproduction of the previous research.*** Correlations of personality factors with total scores are probably more reliable than those with the Unknown and Known scores separately. Comparing the total correlations for the two years it will be seen that they are in the same direction, which is satisfactory so far as it goes; but it will also be seen that the new correlations are much closer to chance than the old. The splendid self-confidence correlation of +.55 in 1952 dropped to +.15 in 1953. The second strong correlation in 1952 was with emotional stability, with $r = +.60$; for Research II it is only +.09. The correlation of total ESP scores and freedom from nervousness, as measured by the Guilford-Martin scale, was +.40, and now for 1953 it is only mildly interesting at +.20. Cattell's measure of ostensibly the same factor, nervousness, now gives a coefficient of almost zero, whereas for the first series it had been —.40. Contrary to our previous view those two tests of nervousness may not be tapping quite the same elements of personality. Freedom from depression (Guilford), which just reached significance in relation to the total ESP scores of Research I, now almost vanishes with a coefficient of +.04. Cattell's factor of worrying suspiciousness

TABLE 2

Correlations of Personality Test Scores and ESP Scores for Research I and for Research II

Personality Factors	Research I			Research II		
	Unknown ESP Score	Known ESP Score	Total ESP Score	Unknown ESP Score	Known ESP Score	Total ESP Score
Guilford-Martin Inventory:						
Factor G, General Activity Level	+.23	—.23	.00	—.14	+.32	+.15
Factor A, Ascendance	—.05	—.02	—.04	—.12	+.27	+.12
Factor I, Self-Confidence	+.44*	+.36*	+.55**	+.03	+.16	+.15
Factor N, Freedom from Nervousness	+.06	+.52**	+.40*	+.25	—.001	+.20
Guilford's Inventory:						
Factor S, Social Extraversion	+.21	+.29	+.34	—.03	+.17	+.12
Factor T, Thinking Extraversion	+.37*	+.10	+.33	+.30	—.10	+.16
Factor D, Freedom from Depression	+.16	+.37*	+.37*	+.16	—.11	+.04
Factor R, Rhathymia	+.43*	—.05	+.26	—.02	+.18	+.14
Factor C, Lack of Cycloid Disposition	—.05	+.38*	+.23	+.03	—.15	—.10
Cattell's 16 P.F. Test:						
Factor A, Cyclothymia	+.03	+.17	+.14	+.13	—.07	+.04
Factor B, Intelligence	+.22	—.06	+.11	+.03	+.08	+.08
Factor C, Emotional Stability	+.60**	+.27	+.60**	—.01	+.12	+.09
Factor E, Dominance	+.26	—.03	+.16	—.20	+.26	+.05
Factor F, Surgency	+.12	—.01	+.08	—.24	+.36*	+.10
Factor H, Adventurous Cyclothymia	+.22	+.16	+.26	—.07	+.22	+.12
Factor M, Bohemian Unconcernedness	—.15	—.06	—.15	—.35*	—.003	—.29
Factor O, Worrying Suspiciousness	—.34	—.29	—.44*	—.09	+.11	+.02
Factor Q_4, Nervousness	—.14	—.44*	—.40*	+.01	—.01	+.003
Cason's Test:						
Annoyance	—.03	—.36*	—.27	—.29	—.04	—.26

For Research I, N is 30 for all correlations except those for Annoyance where N is 36.

For Research II, N is 32 for all correlations except those for Annoyance where N is 35.

One asterisk following a coefficient indicates significance at the 5% level.

Two asterisks following a coefficient indicates significance at the 1% level.

The Research I correlations for Cattell's test differ slightly from those published previously because the Cattell questionnaires have been re-scored by more recent norms (used also in Research II).

which appeared important in 1952, now shows no relation to total ESP scores of 1953. These factors just named were the ones significant in correlations with total ESP scores in 1952.

In the new research, four of the six important correlations bear the same sign as in the previous research. In itself this is not at all exciting, in view of the apparent failure of the pure correlations themselves to attain significance; however, the relative steadiness of the correlation "signs" suggests that though we do not know the true value of the correlations themselves in the population from which our samples were drawn, it can be said that at any rate they are positive.

In the 1953 series, two of the personality factors that appeared unimportant in 1952 show a significant relation to one of the two types of ESP test, although they are not correlated highly with total ESP scores. Cattell's measure of surgency (talkative, frank, alert, cheerful disposition) was positively correlated with the Known type of ESP test ($r = +.36$). The Cattell factor of "bohemian unconcernedness" was negatively correlated with scores on the Unknown ESP test ($r = -.35$), that is, subjects tending to be unconventional, undependable, and imaginative obtained low ESP scores in Unknown runs. Since these two factors showed no strength in Research I, we do not feel it wise to place much emphasis on their emergence in Research II. All that need be said about them now is that they bear watching in future research.

Guilford-Martin's factor of Masculinity-Femininity does not appear in the correlation table because the correlation method is not appropriate for this factor for which men are expected to have high scores and women, low scores. Instead the subjects were separated according to sex and each was rated as being either above or below the average masculinity-femininity score of his or her own group. The results for both men and women were uninteresting. Neither was there any indication of a relation of masculinity-femininity scores to the ESP scores obtained with the two different experimenters handling the cards. For the 1952 Series there had also been no significant relation between ESP scores and this personality variable.

Conjectured Causes of Declines

The unconscious is deceitful above all things and desperately wicked: who can know it?

Jeremiah, mildly adapted.

An experimental finding may be *frequently* but not always reproducible, and the occurrence of one failure may not warrant the

extrapolatory opinion that this settles the issue. However, it seemed wiser to us to conclude that the experiment is not *always* repeatable with so small a number as 30 persons. We then asked: Why in view of the high success of Research I, should Research II prove so disappointing? To get the answer we proceeded to make a close examination of parts of the data, both ESP and personality. In doing this, we were effectively constructing a set of new hypotheses. Those hypotheses are of the *a posteriori* order, and consequently no conclusions can be drawn from them. On the other hand, no experimentalist can hope to be wise before the event at all points in his research program, especially in so intricate a subject as psychical research. It seems all the more desirable to hold an inquest, so to say, on Research II, and ask: Why did the patient die?

It may be conjectured that in Research I we were fortunate to obtain significant results; the significances were too extensive however to be ascribable to chance variation. After the report on the first research had been presented at the First International Conference of Parapsychological Studies at Utrecht in the summer of 1953, more than one friendly commentator remarked that we were "lucky" to have obtained significant correlations from such a small number of subjects. The validity of the correlation results was not questioned, but rather it was judged that significant results from so narrow a range of subjects was surprising. The corollary of this proposition, to which we attach importance, is that we happened to strike upon a particularly potent portion of the population for correlation work at that time, and that this could scarcely be expected to recur so attractively on a second occasion with the same small number of cases. This would imply that psychical gifts and personality variables were in Research I correlated in an exceptionally refined degree, but that this fineness of correspondence between the psychical and the psychological aspects could not be expected to arise very often with *any* group of 30 subjects. If there is a conjunction existing between psychical events and personality qualities (and nearly all the published evidence points in this direction), then the solution to the large question raised by the results of Research II is self-evident. It is simply to increase the number of subjects employed in a research. We ourselves have come strongly to the belief that the number of subjects may be a crucial factor for success.

Prior to the start of Research II it was our intention to investigate at least 36 subjects and at most 60 subjects. It will be seen that we obtained only 35 and those with difficulty. How the picture would have appeared had the maximum number been obtained cannot be known; but it may be confessed that the shortage of subjects was our first, and, on reflection, our largest disappointment.

Now with regard to the percipients of Research II, the important question to ask appears to be: In what respects, if any, did the circumstances and human material of that research distinguish it from its predecessor? The scene of the experiment was different and has already been described. Its influence may have been of a negating nature, but there is no means of telling this. There were more true volunteers in Research I than in II. In the first series many subjects were previously known to us; in the second, none were. In Research I there were two women to every man; in Research II, the numbers were almost equal.

The subjects themselves may be considered under three headings:

1. Their ESP capacities
2. Their personality factors
3. Differences between subjects

Comparison of ESP Capacities

With regard to the ESP capacities of the two groups of percipients we considered only those who returned their personality data (and upon whom, therefore, the correlations depend), and find that in Research I the average run score was 5.07, and in Research II the average run score was 4.84. Using the empirical variance, we find that the difference between the two groups is represented by a t of 2.13, which with 60 degrees of freedom, has the associated probability of .035, a result that would arise by chance only once in 28 pairs of researches of this magnitude. So far as ESP is concerned, our two groups were not *like* each other but *different*. Since ESP is the principal variable against which all personality relationships are reckoned, it naturally follows that correlations in which the mean value of the independent variable was "x," say, would not necessarily be the same as those in which the corresponding mean value was "y," say.[3]

Fortunately the variances of the ESP subjects in the two series were not disparate. Using the residual variance and applying Bartlett's method for the comparison of variances, we found $\chi^2 = .223$, with a probability of .64, indicating that the variances were approximately similar.

Comparison of Personality Factors

The finding that the ESP scores of the two groups were different was soon followed by the discovery of evidence to suggest that their personality characteristics were also dissimilar. Table 3 shows the

[3] For this reason, when pooling the correlations for a later section of this paper, we did not compound them directly by Fisher's method, but determined the general mean for the two series and recomputed the correlation coefficients.

mean scores for 19 personality variables in each series. It will be recalled that high ESP scores were associated with the "socially desirable" qualities, and conversely. In most of the personality tests a high score implies a quality that is advantageous in the social milieu, but for Cason's annoyance test and for several of Cattell's scales the opposite is the case (Bohemian Unconcernedness, Worrying Suspiciousness, and Nervous Tension). In the table we give the actual mean values for the two series, and to indicate whether the degree of social desirability rose or fell in 1953 as compared with 1952, we have introduced a column in which the rise or fall is indicated by the words "Up" or "Down." It will be seen that in 15 cases out of 19 there was a drop in social desirability of the factors in 1953. Few of these differences are significant as reckoned by "Student's" *t*-test, but the consistency of the decline in personality factor strength is too marked to be overlooked.

TABLE 3

Comparison of 1952 and 1953 Means for Personality Factors

Personality Factors	1952 Mean	1953 Mean	Change in Social Desirability
GUILFORD-MARTIN INVENTORY:			
Factor G, General Activity	5.36	4.72	Down
Factor A, Ascendance	5.76	5.22	Down
Factor I, Self-Confidence	5.77	5.13	Down
Factor N, Freedom from Nervousness	5.10	4.97	Down
GUILFORD'S INVENTORY:			
Factor S, Social Extraversion	5.53	5.00	Down
Factor T, Thinking Extraversion	4.47	4.03	Down
Factor D, Freedom from Depression	5.47	5.38	Down
Factor C, Lack of Cycloid Disposition	5.37	5.38	Up
Factor R, Rhathymia	5.40	4.38	Down
CATTELL'S 16 P.F. TEST:			
Factor A, Cyclothymia	4.93	5.56	Up
Factor B, Intelligence	6.73	6.06	Down
Factor C, Emotional Stability	5.73	5.22	Down
Factor E, Dominance	6.13	5.19	Down
Factor F, Surgency	5.23	4.66	Down
Factor H, Adventurous Cyclothymia	6.07	5.31	Down
Factor M, Bohemian Unconcernedness	6.63	5.69	Up
Factor O, Worrying Suspiciousness	5.40	5.56	Down
Factor Q_4, Nervous Tension	5.17	5.00	Up
CASON'S TEST:			
Annoyance	45.75	52.66	Down

Summing up: The ESP scores in Research I were in general positive; in Research II they were generally negative, and the comparison signifies that *different* populations were being investigated.

Concomitantly with the foregoing, there was a fall in the desirable personality qualities in 1953. The net result of the interaction of these two forms of decline may have been the main cause of the heavy drop in the correlation coefficients. This appears to be an important lesson to learn in the guidance of the researcher's steps toward the development of repeatable experiments.

A Group of Aberrant Subjects

Next we inquired whether the watering down of the correlations in Research II was due to subjects in general or to some small number of them. Closest attention was paid to the self-confidence and ESP relationship.

The lowest ESP scores in the experiment were produced by five confident subjects. In general the ESP scores in Research II tended to increase with increased confidence scores as is illustrated in Figure 2. But the gentle upward slope from lower left to upper right in the graph is marred by the group of five low-scoring subjects who had confidence scores of 6 and 7. This group sticks out like the proverbial sore thumb. We were naturally curious about this strange anomaly, unique in both researches.

In the hope of unearthing some clues as to why these confident subjects should have scored so low in ESP (they called 80 runs with a deviation of —60), we studied their data more closely. Before mentioning some of the characteristics noted, we wish to emphasize that the findings are not presented in the nature of proof. Since the hypotheses tested were of the *a posteriori* order, they can be of suggestive value only. (For this reason we omit statements of probability.)

When these five subjects are removed from the picture, the correlation for self-confidence and total ESP scores for the remaining 27 subjects of Research II becomes +.44, which is significant and in good agreement with the +.55 of the previous research.[4]

[4] Removal of the five aberrant subjects from other correlations of the 1953 series brings the correlation coefficients much closer to those that were significantly related to total ESP scores of Research I. The effect of omitting those five subjects is shown in the middle column below, with the total results of the 1953 series and the 1952 coefficients being repeated for comparison on the right and left, respectively.

Personality Factor	1952 Series N = 30	1953 Series N = 27	1953 Series N = 32
Freedom from Nervousness	+.40	+.34	+.20
Freedom from Depression	+.37	+.33	+.04
Emotional Stability	+.60	+.28	+.09
Worrying Suspiciousness	—.44	—.23	+.02
Nervous Tension	—.40	—.29	+.003

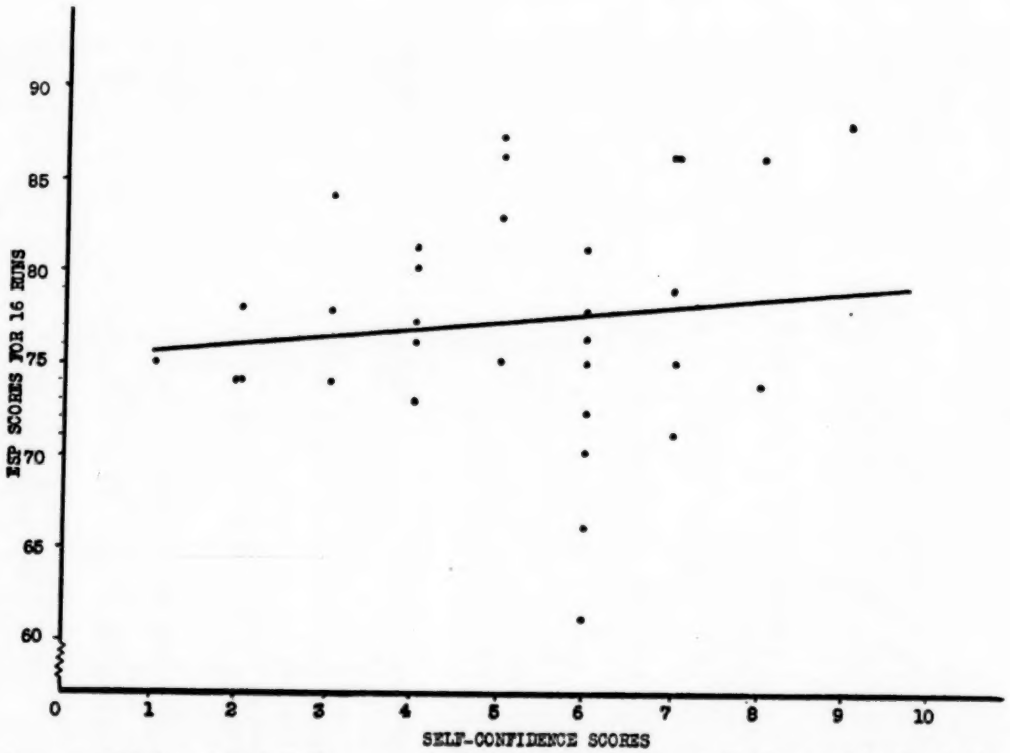

FIGURE 2. Relation of total ESP scores to self-confidence for Research II. The straight line is the regression line which gives the best statistical description of the ESP-confidence relationship. Each dot represents a subject.

In what ways then do the five lowest scoring subjects differ from the others? We noted three differences:

1. Three of the five showed a very strong tendency to confuse two of the ESP symbols. When the target was a Cross, they tended to say Square. When the target was a Square, they tended to call Cross. (These two symbols were adjacent in the row of sample cards, arranged in alphabetical order in front of the subjects during the tests.)
2. The five subjects made their calls at a significantly slower rate of speed than did the other confident subjects. An average of slightly over 7 seconds per call was taken by the five aberrant subjects, while the positive-scoring confident subjects averaged 3.66 seconds per call.
3. The five subjects tended to be much more introverted (Guilford's Factor T) than were other confident subjects.

An introvert is essentially an introspective person, given to inward meditation and the cautious weighing of evidence prior to action. The quality may be immensely profitable in intellectual fields; but, as the evidence from the comparison of our two researches suggests, it may be unprofitable for the production of psychical experiences.

That *time* was occupied in meditation prior to calling might be evidenced, as is the case, by a slow speed of calling. And if *spontaneity* is an accompaniment of successful ESP production, the lack of it may well create confusion as to the right symbol to call, such as has been found among several of the five non-conforming subjects above.

It would be a mistake, however, to conclude that a strong tendency toward introversion is the sole cause of the singular deviations of the five subjects. One other factor may have been more important for two of them. These two subjects were unfortunately confronted with target series which later proved nonrandom. In any long series of randomized cards, the numbers of each kind of symbol will tend toward equality (and in Research II as a whole, they did). But there may be sections within the series in which the equality test will fail. This was the case for two of the aberrant subjects,[5] the nonrandomness in their data being represented by probabilities of .02 and .014. The theory of statistics rests upon the assumption of randomness. Violation of this requirement makes it logically impossible to apply statistical techniques to the data. It follows that the nonrandom cases unhappily discovered in our own research are for that reason *hors concours*. In correlation studies, it would seem advisable in the future to insure series of random targets for each subject so that the target material for all is strictly comparable.

[5] One non-aberrant subject also had a nonrandom target distribution. Her results were undistinguished (negative deviation of 2 for 16 packs); she was unconfident.

Both Researches Pooled

The results of Research II were a great disappointment. The previously significant relationships had provided us with the hope that they would be readily repeatable with *small groups of subjects* —that is, about 30 in the sample. This hope we now abandon. We must reckon in terms of larger numbers.

From the beginning of plans for Research I we have had two objectives in mind:

1. To obtain significant correlations
2. To determine the true value of those correlations

Eliciting significant results as in (1) is an objective in all psychical research and need not be labored now. The second is of a quite different nature. By "the true value of the correlations" we mean the following. We are concerned to find out, not what are the correlations associated with the members we have used as subjects, but rather what are the correlations associated with the *population* (in our case, the collegiate population of the U. S.). We cannot treat the whole population, but we can investigate samples (as has been done twice) and from these samples draw inferences relative to the population. This of course is the established method of making inductive inferences in all departments of experimental science.

To illustrate how very differently the two objectives mentioned above bear on the problems at issue we may give an example. Suppose a team of investigators set out to determine the correlation, if any, between subjects' ESP scores and some other factor. In a long period the team might use a million student subjects, and at length announce that the correlation found had a probability of 1 in 10^{23} (10 followed by 22 zeros). This probability is so astronomically remote from chance as to make us recall the words of Frege on another occasion, "Alas, arithmetic totters." However, we are equally interested in the second objective mentioned earlier and we therefore ask, "What was the value of the correlation?" The answer is

$$r = +.01$$

That is to say, the significance is prodigious and the correlation is paltry and of no utility.[6]

What are wanted are correlations of practical value—ones large enough to be useful in research and ones backed by sufficiently large numbers of subjects to carry conviction. In this latter connection it

[6] When the number of cases is large the correlation coefficient is distributed almost normally with variance $1/n$. Whence in the above example, the normal deviate is $.01 \times 1000 = 10$, which has the probability stated.

may be recalled that the larger the number of people tested, the more reliable (or in statistical parlance "consistent") are the final inferences likely to be. As Professor (now Sir Ronald) Fisher has remarked, "In inductive reasoning we are performing part of the process by which new knowledge is created. *The conclusions normally grow more and more accurate as more data are included*" (6). (The italics are ours.) And, on this theme and in direct relation to the present work, it is instructive to repeat a remark of Snedecor, which, though it is no doubt a scientific commonplace, is useful to mention in a field in which product moment correlations have not frequently been used. The exact correlation called ρ in a population is generally unknown (e.g., in the present case it is impractical to investigate the entire student population of the U. S.), but samples provide estimates of that exact value. "There is often occasion to think," Snedecor comments, "that several sample correlations are drawn from a common ρ. If this null hypothesis is not rejected, then it is appropriate to combine the r's into an estimate of ρ more reliable than that afforded by any of the separate r's" (13, p. 151).

The matter may be summed up in two rather obvious subjunctive remarks: 1) If correlations are not combined we shall never be able to determine the true correlations in the population; and 2) If only significant correlations were combined, the resultant r's would be spuriously large.

Accordingly, to determine to a greater degree of reliability than hitherto possible the magnitude of the correlations, irrespective of their associated probabilities, we now pool the data of the two series. The results of the correlation of personality-test scores and ESP scores for all 62 subjects are given in Table 4.

In the 1952 series alone, it will be recalled, 16 correlations were significant at either the 5 per cent or 1 per cent level. Of that group ten remain significant for the combined results of the two series, as may be seen in Table 4. These may be summarized in words as follows (terms refer to high-scoring ESP subjects):

Unknown ESP Score	Known ESP Score	Total ESP Score
Self-Confidence	Self-Confidence	Self-Confidence
	Freedom from Nervousness	Freedom from Nervousness
Thinking Extraversion		
Rhathymia		
Emotional Stability		Emotional Stability
	Lack of Irritability (i.e., Annoyance)	

TABLE 4

Correlations of Personality Test Scores and ESP Scores for Both Series Pooled

Personality Factors	Unknown ESP Score	Known ESP Score	Total ESP Score
GUILFORD-MARTIN INVENTORY:			
Factor G, General Activity Level	+.05	+.12	+.12
Factor A, Ascendance	−.06	+.17	+.08
Factor I, Self-Confidence	+.26*	+.27*	+.39**
Factor N, Freedom from Nervousness	+.15	+.28*	+.31*
GUILFORD'S INVENTORY:			
Factor S, Social Extraversion	+.09	+.23	+.24
Factor T, Thinking Extraversion	+.35**	+.03	+.28*
Factor D, Freedom from Depression	+.16	+.15	+.23
Factor R, Rhathymia	+.26*	+.11	+.27*
Factor C, Lack of Cycloid Disposition	−.01	+.13	+.08
CATTELL'S 16 P.F. TEST:			
Factor A, Cyclothymia	+.04	+.01	+.04
Factor B, Intelligence	+.13	+.06	+.14
Factor C, Emotional Stability	+.30*	+.21	+.37**
Factor E, Dominance	+.05	+.17	+.16
Factor F, Surgency	−.02	+.18	+.12
Factor H, Adventurous Cyclothymia	+.08	+.22	+.22
Factor M, Bohemian Unconcernedness	−.20	+.01	−.14
Factor O, Worrying Suspiciousness	−.20	−.07	−.20
Factor Q_4, Nervousness	−.05	−.19	−.18
CASON'S TEST:			
Annoyance	−.17	−.24*	−.26*

The number of subjects is 62 for all correlations except those for Annoyance where N is 71. One asterisk following a coefficient indicates that it is significant at the 5% level; two asterisks indicate significance at the 1% level or below.

The six other coefficients that were significant in 1952 fail to stand up to the test of the second series, and the utility of the relevant personality factors as determinants of percipients' ESP scores is open to doubt. Those doubtful personality characteristics are:

UNKNOWN ESP SCORE	KNOWN ESP SCORE	TOTAL ESP SCORE
	Freedom from Depression	Freedom from Depression
	Lack of Cycloid Disposition	
		Worrying Suspiciousness
	Lack of Nervousness	Lack of Nervousness

There *may* be some true correlation between these factors and the ESP scores of the college student class in general, but if so, it is evidently weak and at the present stage unestablished.

On the other hand, three correlations that were suggestive of some relation with ESP in 1952 found enough support in the second series to remain in the domain of significance for the combined results. Those new effects are all correlations with total ESP scores. The factors concerned are: Thinking Extraversion, Rhathymia[7] (happy-go-lucky, carefree disposition) and Annoyance (the less annoyable a person, the more likely his ESP score to be high, and conversely).

In brief then we have the following significant correlations between the ESP scores and personality factors of 62 subjects, except for the annoyance score data where the number is 71:

Unknown ESP Score	Known ESP Score	Total ESP Score
Self-Confidence	Self-Confidence	Self-Confidence
	Freedom from Nervousness	Freedom from Nervousness
Thinking Extraversion		Thinking Extraversion
Rhathymia		Rhathymia
Emotional Stability		Emotional Stability
	Lack of Irritability (i.e., Annoyance)	Lack of Irritability (i.e., Annoyance)

With regard to Cattell's Emotional Stability scale, we feel that, notwithstanding the significant correlation for the pooled series, the influence of this factor is less satisfactory than most of the other correlations. When the correlations of emotional stability with total ESP scores for the separate researches are compared, we obtain a normal deviate of 2.26 and an associated probability of .024, which indicates a real difference between the emotional stability correlations of the two years. While for the sake of completeness in Table 4, we have combined the emotional stability coefficients for the two years, the result must be viewed with reserve. For Unknown ESP scores and emotional stability relations, the difference between the two series is also significant, but for the Known ESP data they were found to be similar in the statistical sense.

Three other correlations are significantly different for the two years at the 5 per cent level. These are: Freedom from Nervousness with Known ESP score, General Activity Level with Known ESP, and Lack of Cycloid Disposition with Known ESP. The last two

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[7] In the report of the 1952 Series it was pointed out that the correlations of Unknown and total ESP scores with Rhathymia were due mainly to three subjects at the highest point of the Rhathymia scale. The fact that the Rhathymia correlations are significant for the combined 1952 and 1953 series is still due to these few subjects. Therefore the general usefulness of this personality factor remains in doubt.

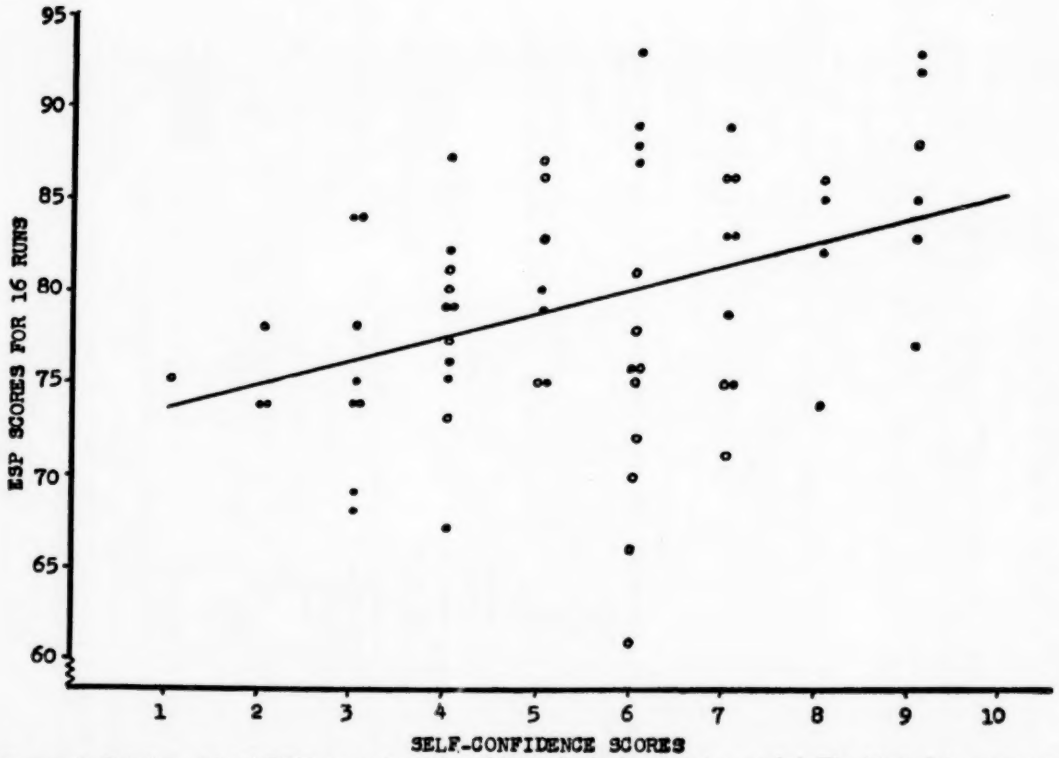

FIGURE 3. Relation of total ESP scores to self-confidence for both researches pooled. The black dots represent the percipients of Research I, and the circles, those of Research II. The straight line gives the best statistical description of the ESP-confidence relationship.

factors have not been of any interest in these researches, but the Nervousness factor is curious and may be worth further investigation.

However, it will be observed that there are 57 correlations for each year, and hence 57 possible comparisons could be made for the detection of inconsistent results. From the above remarks it will be clear that only five pairs are inconsistent and 52 pairs are consistent.

In our previous paper on this subject, we attached the greatest importance to the self-confidence factor as an influence on ESP scores. The correlation there reported for total ESP was +.55. In Research II it is +.15, and for both researches together it is +.39. This last figure, since it contains the work of a greater number of subjects than either series alone, must be regarded as the best estimate of the true correlation pertaining to college students' self-confidence and their ESP abilities. The correlation of +.39 is such as would occur by chance only once in about 550 researches of this size.

Figure 3 shows the relationship between self-confidence and total ESP for the pooled series. The black points represent the percipients of Research I, and the circles, those of Research II. The sloping line describes the general tendency of ESP scores to rise directly with the increase of subjects' self-confidence scores. The regression coefficient (b) has the value of 1.31; that is, for each one-point improvement in self-confidence, the ESP score increases *on the average* by 1.31 hits.

Considered separately, the correlations of self-confidence with Unknown and Known ESP scores are almost exactly the same (+.26 and +.27, respectively) and are both significant. A feeling of self-confidence in a percipient seems to enable him to exercise his ESP skill very well in the standard Unknown ESP test; and this same factor in his personality appears to enable him to surmount the more intimidating situation represented by the Known ESP test.

The influence of degrees of emotional stability as measured by Cattell's test is distinctly puzzling. As has been shown, for the two researches the correlation coefficients for this factor and Unknown ESP scores are significantly different from each other. It is therefore questionable whether they should have been combined as was done in Table 4. On the other hand the two correlations of emotional stability with Known ESP, though small, are closely comparable and, when the two researches are combined, we have a coefficient of +.24 which fails to achieve the 5 per cent significance level by a hairsbreadth (that level arises at $r = +.25$). One would say of a result like this only that it ought to be kept in mind in future research. It would certainly be interesting and credible on general

psychological grounds if it were shown by using larger numbers of people than here reported that those who are blessed with emotional stability were most likely to score high in ESP tests.

For the correlation of total ESP score with emotional stability, we have a coefficient of +.37, a result that would occur by chance about once in 350 investigations of this size. This figure, however, must be regarded with scepticism, since it involves the conflicting correlations of this factor with Unknown ESP for the two series. On the whole we incline to the view that emotional stability does promote the higher scores in ESP but that the strength of its influence cannot at present be described with accuracy.

Somewhat similar remarks apply to the less striking element of nervousness. Guilford and Martin's test of "Freedom from Nervousness" was significantly related to total ESP scores for the combined series; however, Cattell's test, ostensibly measuring the same factor, is of uncertain value since the significant results of Research I were not repeated the following year.

Thinking extraversion as related to ESP is one of the most pleasing results we have to show. Whereas this factor shows no relation to Known ESP-test scores, the picture is quite different for the Unknown or standard ESP situation. Here the percipient is relieved of the disheartening experience of seeing call by call how often he is wrong; and now the virtue of thinking extravertly becomes clear. In Research I the correlation was +.37; in Research II it was +.30, and the combined result is +.35, an outcome that would occur by chance only once in 200 such experiments. We thus find at the present stage of the investigation that when subjects are kept in ignorance of their successes and failures until the end of the run, those who are given to "thinking outwardly" may be expected to attain a relatively large number of paranormal cognitions, while those who "think inwardly" (addicted to introspection) will tend to fail in such an ESP test. When the data from both kinds of ESP tests are pooled, the correlation with this factor is +.28, which is fairly close to the 2 per cent level of significance; but this correlation consists almost entirely of the striking effect in Unknown card-calling data with virtually none in the Known data.

A pleasant surprise is the apparently confirmatory correlation arising from the use of the modified form of Cason's Annoyance Test. For each research the annoyance scores were correlated with the total ESP scores with the following results:

	Research I	Research II	Total	P (Total)
Coefficient:	—.27	—.26	—.26	.026

The breakdown of the figures for the annoyance correlation with the Known and Unknown ESP tests separately is by no means so neat as for the ESP totals shown above. Even so the similarity of the effect shown in the two tests, separated by a year, is too interesting to be ignored.

The combination of the two series may be summed up with the following conclusions:

1. The correlation coefficients for the two researches show a good deal of variation, but in most cases (52 out of 57) these variations are not significant.

2. Consequently the exact correlations that would be true of a large college population (as distinct from the relatively small groups discussed in this report) are not easy to arrive at in the absence of more wide-sweeping research. Estimates of the size of correlations to be expected will be given in later paragraphs.

3. The combined results of the two series provide the best estimates of the correlations between personality factors and ESP that would be likely if the tests were repeated among the same type of young men and women.

4. The main finding is that self-confidence is the strongest personality factor affecting ESP scores. It is significant for the Unknown and Known tests and for total ESP scores, the correlation associated with the last mentioned having a probability of 1 in 550.

5. The evidence favoring the hypothesis that ESP is a correlative of three other elements is good. Those elements are: Freedom from Nervousness, Thinking Extraversion, and Low Irritability Level (annoyance).

The Problem of Repeatability

Let us be clear what we mean by "repeatability." The foundation of scientific knowledge is repeatability of experimentation. To consider a classic example of repeatability, Robert Boyle claimed that the volume of a gas varies inversely as the pressure. He demonstrated this to his own satisfaction by his well-known experiment employing a U-tube, air, and pressure obtained from mercury. Other researchers in the intervening three centuries have repeated the experiment and obtained approximately similar results. And so it is through the whole range of experimental science. Lacking repeatability of results, no experimental science would exist, only chaos.

Boyle took a *sample* of air. Other scientific workers drew samples of air and, obtaining similar results, were able to apply the logic of inductive inference, arguing that what was true of their particular samples of air was true of air in general. They argued *from the*

sample to the population. The absence of repeatability in psychical experiments, and hence the logical impossibility of making general inductive inferences (as conceived in all other branches of science), has crippled the progress of psychical research and constitutes the one valid excuse for the general attitude of indifference of scientific workers to the subject.

In relatively simple sciences such as pure chemistry and physics, repetition is easy to obtain. In biological fields the situation is quite otherwise. In psychology failure to repeat a significant result is not uncommon; in psychical research failure is the order of the day (with one possible exception discussed below).

In Research I we investigated the ESP responses and the personality attributes of thirty young men and women, college students for the most part. Highly significant correlations were obtained, and these correlations and the conclusions drawn from them remain valid at the present day. The question then arises: "Is what has been found true of 30 persons true of *all such persons* in general?" This exemplifies the main contemporary problem in psychical research.

Now, if it is the researcher's aim to extend the range of discourse from particular cases (the samples) to the general field (the population), certain requirements must be met. He cannot investigate every member of the population that interests him (there are about 2,000,000 university and college students in the United States). He can instead draw samples whose characteristics are typical of the population. If successive samples are similar to each other and to the population, it will be in order to argue that what is true of the sample is true of the whole population. The whole matter will have reached a stage of wide generalization and the experimental results will be repeatable.

In the event of the sample not being representative, a very different situation arises. The *Literary Digest* poll of 1936 was in error in predicting a Republican victory in the Presidential election because it drew its sample from lists of telephone subscribers and automobile owners who, in the political realm, were not representative of the voters at large.

The results of each of our researches are true so far as they go, but in our view they do not permit us to argue from either sample to the population and say that the findings of Research I (or of Research II) are likely to be true of the personality-ESP correlates of undergraduates in general. We cannot say this, and consequently we cannot say that repeatability has emerged. What can be said is that, having disclosed some of the harder obstacles to repeatability,

we are in a more favorable position to proceed further towards the construction of the repeatable experiment.

Whately Carington was the first to point urgently to the need for repeatability in psychical research. All his later researches were directed towards that end, and his last experimental report was entitled "Steps in the Development of a Repeatable Technique," published in the Society's *Proceedings* (1). This paper contained an account not of experiments conducted by him but by 25 independent experimenters in the United States and Great Britain working under his advice and distant supervision. The stimulus material was not cards but freehand drawings of which there were ten in each experiment. The *total* result was highly significant ($P < 10^{-6}$, about one in a million against the chance hypothesis), though the majority of the experiments were not singly significant. Surveying the whole effort, Carington concluded that "the average experimenter" should obtain results at the .05 level of significance if he recruited approximately 79 subjects and used ten stimulus drawings. It is however a remarkable fact that none of his collaborators employed as many as 79 subjects, the actual numbers varying from 1 to 52.

Carington's work may be assessed in another light, a light more favorable, we believe, than that which he himself used. He reported 42 independent experiments. From his Table II (1, p. 56) it is not difficult to compute that nine were significant at the .05 level—that is, nine out of 42 cases. This is on the assumption that the distributions were normal. From whatever direction they are considered, there seems no reasonable doubt that Carington's results did provide evidence (stronger perhaps than he asserted) that his findings were reproducible and had been reproduced *by others* to a significant degree.

Carington did not claim that his technique would provide repeatability *in every case*. The evidence is that significant repetition might be expected nine times in 42 experiments; in other words about once only in four experiments. In view of the precariousness of all psychic knowledge it would be optimistic indeed to fancy that reproduction of results in *every* experimental case is in prospect at the present time. Such things do not happen in psychology or the other sciences of life. It would seem presumptuous to suppose that any psychical experiment will be unfailingly reproducible in its results at the present time. Such a prospect would imply that psychical research has attained the predictive exactness of experimental physics and pure chemistry; and there seems no reason to believe this. Possibly about one success in four (as in Carington's work above) is about the best one may expect in psychical research at the present time. This is not much, but it would be a big step

forward all the same. In view of the *failure* of the Carington experiments in about three cases out of four, it is accordingly not surprising—and possibly to be expected—that we have failed in one case out of two.

Carington's work brings to mind consideration of another aspect of quantitative research. Dr. Gardner Murphy has reminded us that ESP scores themselves appear to be *unreliable,* in the sense that, using only ESP cards, you may get a significant result on one occasion but there is no guarantee that you will get a similar result on subsequent occasions. Dr. Murphy's suggestion is that this inconsistency may be inseparable from the use of stimulus material in which the probability of a hit is as large as 1/5, and that targets with smaller probabilities might provide more stable results. The ratio of ESP hits to chance hits would rise and, by reducing the proportion of chance elements in ESP-test scores, greater reliability may be achieved.

The great success that attended the introduction of cards with high probabilities may have dimmed our vision to the virtues of cards, drawings, or other objects with *low* probabilities. Old friends, like old shoes, are not willingly discarded, but it may be that the standard ESP cards because of the erratic nature of the results that come from their use may give place to more sensitive materials. The subject deserves study.

Probable Correlations to be Expected in Future Research

What sort of correlation results between ESP and personality factors may be expected in future research? We can predict these on the basis of the total results gathered to the present time. It has been stated earlier that the *exact* correlations to be expected from the population (roughly speaking, all college students in the country) are difficult to arrive at. To determine them with certainty is impossible without examining the entire population. Fortunately we are able to do the next best thing, which is to ***estimate the limits between which such correlations would be expected to lie in repeated sampling.***

No one endeavoring to reproduce the work reported in these papers need expect to obtain such a self-confidence-ESP correlation as +.388 precisely. But it seems natural to ask, within what range will correlations from repeated sampling fall? Would it be surprising to get a correlation of +.50 in one sample and, say, +.20 in another, and so on? The answer to such inquiries takes us away from the pursuit of "significance" and concentrates attention on the

value, one might say *quality,* of the correlation itself. To do so is to take a step, however tentatively, towards the eduction of laws in psychical research.

The limits within which a correlation may be expected to fall in repeated sampling are given in the usual statistical texts, as for example Cramér (5, p. 467) or Snedecor (13, p. 150). Following these methods, we find that:

> *If groups of about 62 subjects at a time are drawn from the specified population, the correlation of self-confidence with ESP will lie for 19 such groups out of 20 between the limits*
>
> $$r = +.153 \quad \text{and} \quad r = +.581$$

In one case out of 20 the correlation will fall outside that range. All this assumes random sampling. We regard the passage in italics as being, on the whole, the most important result of these experiments to this time, since it incorporates valid inference with cautious prediction.

It is reassuring that the limits of predicted r's are confined to positive correlations, the lower limit does not touch negative correlations. The same reassurance emerges, though less markedly so, from those other significant correlations with total ESP scores already mentioned. (The negative signs attached to Cason's Annoyance are equivalent to the positive signs found elsewhere in the table; that is, the "desirable" qualities are related to positive ESP scores and conversely.) They are given below:

Range of Correlations Expected from Repeated Sampling

Personality Factor	Observed r (N = 62)	Expected Limits of r
Self-Confidence	+.39	+.15 to +.58
Nervousness (Guilford-Martin)	+.31	+.07 to +.52
Thinking Extraversion	+.28	+.03 to +.50
Rhathymia	+.27	+.02 to +.49
Emotional Stability*	+.37	+.13 to +.57
Annoyance	—.26	—.01 to —.48

* Subject to the reservations given on an earlier page.

The Long Range Aim

Personality associations with paranormal cognition have been much in evidence for a dozen years, so there was little need to add further proof. The objectives here were different. They were an endeavor to advance a few steps forward on previous findings—not to seek more "proof," but to find methods of reproducing the phenomena.

In this last regard we find ourselves in close accord with some expressions of view about extrasensory perception written by Sir Ronald A. Fisher some sixteen years ago, and read to the annual meeting of the American Psychological Association (12):

> Perhaps I may say, with respect to the use of statements of very long odds, that I have before now, criticised their cogency on the grounds, not only that the procedure of calculation is often questionable, but that they are much less relevant to the establishment of the facts of nature than would be a *demonstration of the reliable reproducibility of the phenomena.* (Italics ours.)

We have found that the expansion of interest from the experimental field in which one tries simply to "get results" into that rather different country in which one tries to advance in steps towards repetition of the phenomena requires also a change in the experimenter's outlook. To consider one aspect: he is no longer concerned with the production of striking significances, which has been done often in the past but has no bearing on the new problems. When a result is "significant" in the purely experimental field, one asks "Significant of *what?*" In most published quantitative work the answer can be given negatively in terms of the null hypothesis, and positively in terms of "clairvoyance" or "telepathy," and no more need be said since the researcher's task is then at an end. In the search for elements of repeatability the question "Significant of what?" does not alone meet the needs of a continuing research. For the experimentalist now has from time to time to *make new decisions.* When a probability is computed, the question raised is, "What are you going to do about it?" For the separate results of one experiment must be judged and decisions made with a view to either rejection of the particular subject matter or special study of it in the next experimental scheme. In some instances the decision is easy to make. Thus, after the apparently striking success of self-confidence in Research I, the decision was made to apply close study to this relationship both in Research II and in our inquiry into the subjects' response to a successful hit, already reported (the "checking" effect, 9). Contrariwise, in Research I the ascendance factor was close to chance ($r = -.05$ and $-.02$) for the two ESP tests. The decision was to pay scant attention to it thereafter since there was no good evidence of its being an ESP discriminator. Similarly with other elements in the experimental picture: we accept or reject one or another, expand or modify our interest as the data may determine.

In the event of the investigation continuing, main attention would be directed to self-confidence. It appears to be linked with ESP in at least four aspects—Unknown ESP, Known ESP, speed of calling

(suggested by the results of the aberrant subjects), and the "checking effect" (the feeling of success). Of the other factors, the following are significantly associated with ESP and therefore invite more penetrating inquiry than we have hitherto accorded them: Freedom from Nervousness, Thinking Extraversion-Introversion, and Annoyance. Of these, the promise of Thinking Extraversion-Introversion has already been examined in this paper, and we believe that in its introspective elements the factor may prove to be one of the causal sources of hitting the wrong target and of confusing one symbol with another. This odd phenomenon of (unconsciously) knowing the truth but speaking falsely has caused much frustration to workers in the field.

Those decisions are easy to arrive at since they stand on the level of statistical significance. But less clear-cut matters are far less easy to decide. Computational work in this type of research is not inconsiderable; in the two experiments reported some 700 product-moment correlations have been assessed (some of preliminary nature or as a check on work already done). Hence it is advisable not to increase the labor by the incorporation of factors that may turn out to be of little practical value to the long range aim. On the other hand, it has to be recognized that a correlation, non-significant in a small sample, may be of great significance—statistically, literally, and psychologically—in the long run as part of the picture of the population. To disregard it might be to discover, hereafter that "the stone which the builders rejected is become the head stone of the corner." We believe that the following ESP correlations, insignificant in the sample, are of continuing interest: Social Extraversion ($r = +.24$), Freedom from Depression ($r = +.23$), Adventurous Cyclothymia ($r = +.22$), and Worrying Suspiciousness ($r = -.20$); and also speed of calling.

All those factors form stable elements in the experimental situation, brought to the scene by the subject himself. There are also transient influences which may modify the effects of personality on the demonstration of the paranormal occurrences. Those influences include the experimenters themselves, the research room, the nature of the experiment (whether it is intrinsically interesting or otherwise), and a variety of others.

All such factors and influences integrate with each other to form in part "the web of thought and action" in human life. Apparently they influence the creation of psychical experiences, and it is conceivable that the psychical factor influences *them.* The aim is no doubt a distant one, but it would be a matter of uncommon interest to investigate and if possible to find how the psychical life is influenced by the psychological, *and conversely;* and in addition,

how those two aspects operate together to produce the common events of life.

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The Cambridge Conference on Spontaneous Phenomena[1]

CAMBRIDGE (England).—Psychical researchers from ten countries of Europe and the Western Hemisphere pledged themselves to organized international cooperation at the conclusion of a Conference on Spontaneous Phenomena, held at Newnham College from July 11 to 17, 1955.

The Conference was organized by the Society for Psychical Research, London, in cooperation between Mr. W. H. Salter, Honorary Secretary of the S.P.R., and Dr. Gardner Murphy, General Research Consultant of the Parapsychology Foundation. The Society's President, Mr. G. W. Lambert, acted as President of the Conference; Mrs. Eileen J. Garrett, President, the Parapsychology Foundation, was President of Honor of the Conference.

The Conference endorsed the "preparation of an international plan looking towards better studies of spontaneous cases." Among the phenomena considered were hauntings, poltergeist phenomena, telepathic dreams, apparitions, and similar happenings in various parts of the world.

The delegates also resolved that "discovery, careful sifting, authentication and intense study of a large number of cases, including recent cases," should be undertaken on a world-wide scale. The Conference laid tentative plans for the establishment of a world center that would serve as a depositary of well-documented cases; no specific center was selected by the Conference, as such a decision is expected to be made at a later date.

Resolution On Methods

Delegates appointed a committee to study "traditional methods of collecting, evaluating and interpreting material," in order to determine whether such methods "have a dependable parapsychological aim and a good psychological, logical, and heuristic basis." The Conference approved continuance of programs carried out by international correspondence on such matters as "E.S.P. Projection," also known as out-of-the-body experience. The Conference also decided to create an international "follow-up" committee to maintain international communications in the field of research into spontaneous phenomena.

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[1] Reprinted by kind permission of the Parapsychology Foundation from its *Newsletter*, July-August, 1955.

The Cambridge Conference continued work begun in 1953 at the First International Conference of Parapsychological Studies in Utrecht, the Netherlands. In 1954, two related conferences took place at St. Paul de Vence, France; these dealt with the relationship between philosophy and parapsychology, and with unorthodox healing.

Delegates and observers to the Cambridge Conference came from Denmark, France, Germany, Haiti, Italy, the Netherlands, Norway, Switzerland, the United Kingdom, and the United States. (A full listing may be found at the end of this report.) As both the Conference site and the living quarters of the delegates were at Old Hall, Newnham College, the meeting provided a unique opportunity for personal contact and the exchange of information and views.

Possibly the greatest interest was aroused by a paper delivered at the very close of the Conference by Dr. Louisa Rhine on "Some Results of the Case Studies at Duke University." The discussion which preceded the Conference's resolution on methods reflected lively concern with the problem of satisfactory selection, authentication and evaluation of cases in the field of spontaneous phenomena.

The Conference began on Monday, July 11, with a short reception, at which Mr. Lambert and Dr. Murphy welcomed the delegates. On Tuesday Prof. Price, reading a brief paper of his own, introduced Dr. Murphy who spoke on "What Contribution to Psychical Research Can be Made Through the Investigation of Spontaneous Cases?" He urged delegates to concern themselves with the need to obtain fresh cases, to recruit and train field researchers, to systematize and organize material, and to obtain effective world-wide collaboration. Mr. W. H. Salter spoke on "Phantasms of the Living and the Dead: The Traditional Method of Research." He described spontaneous phenomena as, of all psychic phenomena, providing "the most valuable contribution to the understanding of the human personality."

On Wednesday delegates heard Prof. Hart's paper, "The Experimental Approach, With Special Reference to Traveling Clairvoyance"; the speaker described phenomena of "E.S.P. Projection," within the framework of his own intensive studies. Prof. F. J. M. Stratton, speaking on "Haunts and other Localized and Iterative Phenomena," provided a variety of illustrative case history material.

"The Psychology of Spontaneous Cases" was discussed on Thursday by Dr. Meier and Dr. Servadio. Dr. Meier provided psychological background to spontaneous phenomena, using techniques of evaluation based on the concepts of Dr. C. G. Jung; Dr. Servadio examined phenomena with a view toward individual "submersion into a less- or a non-individualized unconscious psychic world." Prof. Ducasse examined "Method in the Investigation of Spontaneous

Paranormal Phenomena," including the possible significance of phenomena "concerning the structure and latent capacities of the human personality and of the paranormal forces or agencies which impinge upon it."

On Friday Mrs. K. M. Goldney, speaking on "The Practical Investigation of Poltergeist Cases," related her experiences in efforts to follow up individual cases that had come to the attention of the Society for Psychical Research. Mrs. Allison, within the framework of a paper on "Some Poltergeist Cases in America," gave a historical survey of such cases in the United States. Mr. Lambert then gave delegates an opportunity to acquaint themselves with his hydrogeological hypothesis regarding poltergeist phenomena, submitting the view that many of these phenomena may be attributed to the tides and other fluctuations in the movement of underground waters.

The evening of Friday was devoted to a lecture by Dr. J. B. Rhine on the question "What Use Can Parapsychology make of Spontaneous Case Material?" and to Mrs. Rhine's lecture, already noted above. Dr. Rhine delineated spontaneous and experimental material; he noted that "just as the general public has been sustained in its interest in the rather technical investigations of the laboratory largely by its familiarity with spontaneous happenings, so the laboratory worker himself can gain a certain supporting effect from firsthand knowledge of these far-ranging human experiences, evidently closely bound up with the findings of his investigations."

Mrs. Rhine, in discussing case study results at the Parapsychology Laboratory, Duke University, stated that her collection and classification of a very great number of cases permitted, in many instances, an insight into "a truth about human nature." She observed that "factors which determine the form of a given case must be those inherent in the individual personality." Thus, she suggested, "the percipient creates his own experience," as for instance, within a telepathic pattern, "based on the distant event, but created to fit his own assumptions, and therefore the relationship between experience and event in extrasensory perception bears little resemblance to the analogous one of sense perception."

Saturday was devoted to business meetings, the drafting of resolutions and farewell addresses. The Conference officially closed on Sunday, July 17, with the departure of the delegates from the city of Cambridge.

List of Delegates

Following is a list of delegates who attended the Cambridge Conference on Spontaneous Phenomena:

Denmark: Mr. Aage Slomann; France: Mr. Robert Amadou, Mr. François Masse; Germany: Dr. Gerda Walther; Italy: Dr. Emilio Servadio; Netherlands: Prof. W. H. C. Tenhaeff, Mr. George Zorab; Norway: Prof. Thorstein Wereide; Switzerland: Dr. C. A. Meier.

United Kingdom: Prof. C. D. Broad, Mrs. K. M. Goldney, Mrs. R. Heywood, Mr. G. W. Lambert, Prof. H. H. Price, Mr. W. G. Roll, Mr. W. H. Salter, Prof. F. J. M. Stratton.

United States: Mrs. E. W. Allison, Mrs. L. A. Dale, Prof. C. J. Ducasse, Prof. Hornell Hart, Prof. Gardner Murphy, Dr. J. B. Rhine, Dr. Louisa Rhine.

Attending individual sessions were Dr. R. H. Thouless (U.K.) and Dr. D. J. West (U.K.).

Attending the Conference as observers were Mr. A. D. Cornell (U.K.), Mr. Martin Ebon (U.S.A.), Miss I. Jephson (U.K.), Dr. Henry Margenau (U.S.A.), Dr. Louis Mars (Haiti), and Dr. D. C. Russell (U.K.).

Also present were Mrs. Amadou, Mrs. Lambert, Mrs. Mars, Mrs. Salter, Mrs. Servadio, Mrs. Wereide, and Mrs. Zorab.

Mrs. Eileen J. Garrett (U.S.A.), delegate and President of Honor of the Conference, was unable to attend sessions for reasons of health, but maintained close contact with the proceedings.

Reviews

NEW DIMENSIONS OF DEEP ANALYSIS: A Study of Telepathy in Interpersonal Relationships. By Jan Ehrenwald, M.D. Pp. 316. Grune and Stratton, New York, 1955. $3.75.

During the past two decades a new type of spontaneous paranormal occurrence has presented itself to the attention of students of psi, the telepathic incident in psychoanalysis. Phenomena previously seen, apparitions, premonitions, travelling clairvoyance, and such, have usually been associated with emotions of considerable poignancy and recollected haphazardly by people untrained in scientific observation and recording. On the other hand, these dreams or pieces of behavior, by which a patient reveals telepathic awareness of events in the analyst's life and thoughts, may take place with only a slight increment of affectivity and are noted with due professional care and accuracy as a matter of routine. Until now, such happenings have only been reported in psychiatric and parapsychological journals, or talked over by a small group of like-minded doctors, but here they are offered for the first time to the general public. For this reason, *New Dimensions in Deep Analysis* deserves a place on the shelf alongside such volumes as *Phantasms of the Living* and *Modern Experiments in Telepathy*.

Here the reader will find accounts of cases which illustrate common elements between therapist and patient, or "tracer effects" that denote a telepathic factor in a dream. More importantly, they demonstrate how telepathic communication occurs, in either direction, in situations where the configurations of unconscious desires and defenses happen to coincide in both agent and percipient, or when the responses to different life situations are similar though largely unrecognized.

Thus, a woman patient of the author's wished to become part of his household at a time when he himself was rejoicing in the establishment of a new and, he hoped, a permanent residence. A male patient's injunction against trespassing on forbidden ground, an incest prohibition, appeared at the same time that the author was troubled because a father surrogate, a landlord, forbade his venturing on a desirable plot near the summer home he thought of renting. One patient's castration anxiety resulted in a precognitive dream; yet others vied in the production of material perceived extrasensorily in order to gain the interest of the therapist whom they shared.

These instances of telepathy are shown integrated so well with the matrix of the dreamer's life and personality and as corresponding so closely with the therapist's concerns that they appear inevitable and comprehensible rather than astounding.

But psi, according to the author, is operative in many phases of interpersonal life, other than analysis, without making itself manifest by tracers. He cites the fact of empathy, that state of mind in which we project our consciousness into that of someone else, believing we can understand his thoughts and emotions because of their similarity with our own. He describes enkinesis, which occurs when our actions correspond with what we consider the drives and needs of another. Then there are the mental and physical closeness of mother and child; the reciprocating patterns of behavior found in neurotics, an inevitable factor in the choice of a mate, which is often ignored; the social pressures which result in the superego and in the existence of the oedipal conflict; and the multiple causes of group cohesion.

After a consideration of the fact that the perception of extrasensory material contradicts the established neurological data on cerebral localization of sensation and motor origins, the book fulfills the promise of its title and indicates how an awareness of psi phenomena can affect the practice of psychoanalysis. The author makes the disquieting inference that some of what a therapist finds in a patient's mind has been telepathically implanted there by himself. This is exemplified by instances of patients whose dream content has varied with the prevailing ideas of the schools of their analysts. The author weighs the advantages and disadvantages of revealing the presence of psi phenomena to the patient, but otherwise his review of the therapist-patient relationship is essentially that of orthodox psychoanalysis.

In evaluating the inferences and theories which the author elaborates from clinical material, this reviewer is of two minds. Speculation about the nature of psi is not only useful for the production of ideas, it is essential if this study is not to remain a mere cataloguing of marvels. The kind of thinking that the author shows here will probably be a spur to the investigation of psi, similar to the stimulus that the infant science of chemistry received from the concept of phlogiston, even though this concept did not endure.

However, since the phenomena under investigation threaten the equanimity of many conservative thinkers, and occur unpredictably and often somewhat incredibly, they demand a particularly convincing kind of discussion. Here the book seems to fall short of what the author could do, due to some extent to his readiness to make unwarranted assumptions. For instance, in discussing empathy, mother-child ties, and so forth, his general theme is that the clues by which emotions and ideas are transmitted consciously from one person to another are too meager and scanty to account for the magnitude of their effects. There must be then a concomitant psi factor. This

reasoning seems unjustified, although the reviewer is ready to accept the conclusion to which it leads, i.e., that the operation of psi in everyday human life is underestimated.

Again, the author has been beset by the difficulty that confronts all those who would delineate psychological novelties, the difficulty that language was primarily coined for what is material and here the subject is something immaterial and inferred rather than perceived. Such entities can only be described by metaphors or diagrams. The figures of speech used by the author are reminiscent in their clarity and richness of the famous similes of Freud. However, the danger is that the more vivid and applicable a metaphor is, the more it beguiles those whose use it into considering it as substance, as the entity itself instead of a mere allegory of some of the subject's aspects. Among other instances, in the comparison of the inception of heteropsychic material with the metabolism of food, it seems that the author has misled himself in this way. However, the main cause for disagreement with the author is that here he seems to have underestimated the grandeur of his subject. The existence of psi is a threat to accepted ideas of causality, of logic, of physical dynamics; it is the promise of a new heaven and a new earth. However, the impression the book leaves is that psi is a part of the personality that can be understood and dealt with by some slight extension of our present knowledge of psychotherapy and neurology. Yet this is probably far from the author's intention in presenting this erudite and well-expressed study.

GERALDINE PEDERSON-KRAG, M.D.

IMMORTALITY: The Scientific Evidence. By Alson J. Smith. Pp. 248. Prentice-Hall, New York, 1954. $3.00.

The author of this book, Mr. Alson J. Smith, is a believer in the Christian doctrine of immortality. Conversant with the literature of psychical research, both qualitative and quantitative, he considers its bearing on that doctrine and is constrained to say that "Man is immortal." Because he looked at the phenomena of psychical research in the light of his previous studies in parapsychology, he points out that his conclusion was reached from the scientific rather than from the psychical side.

The author's conclusion, however, can hardly be regarded as scientific. It was not arrived at by the method one would expect from a scientist. Personal belief in survival, in whatever form, is one thing; scientific proof is quite another matter, and whether such proof will ever be obtained remains to be seen.

Apart from the above remarks, *Immortality* offers an interesting survey of some highlights in psychical research and parapsychology. In the opening chapters the author sums up what he considers the best types of evidence for survival, including a survey of the trance phenomena of such noted sensitives as Mrs. Piper, Mrs. Garrett, Mrs. Leonard, and Mrs. Soule.

Mr. Smith's summaries of cases sometimes reveal a lively imagination rather than adherence to the facts. The origin and development of the famous "Lethe" cross correspondence are hopelessly confused. In the section on "Book Tests" one intriguing example (p. 99) seems paradoxical. (Unfortunately there is no reference.) The test was given to Sir William Barrett in 1921 with Frederic Myers as the alleged communicator. The room indicated by the medium (Mrs. Leonard) was in the Barrett house in Devonshire Place, London. The author tells us that Mrs. Leonard had never been in the Barrett home but "Myers *had* been in the house many times." The implication is that the source of the communications was "Myers' " memory. But since Myers died in 1901 and Sir William was Professor of Physics at the Royal College, Dublin, from 1873 to 1910[1] does it seem probable that he occupied the London house before Myers' death at the turn of the century? I have found no reference to the case in the S.P.R. Index. Presumably the source of Mr. Smith's summary of the book test is in Sir William's Introduction to Drayton Thomas' book, *Some New Evidence of Human Survival* (pp. XIV-XVI). From this source I gather that Myers was familiar with Sir William's early life rather than with the house in Devonshire Place.

Chapter 7 traces the development of parapsychology with emphasis on the experiments at the Parapsychology Laboratory at Duke University, under the direction of Dr. J. B. Rhine, and the experimental investigations of Dr. S. G. Soal and Whately Carington in England.

The chief virtue of *Immortality* lies in its appeal to the general reader who may be stimulated to take an active interest in a field of growing importance which is bound, in the course of time, to receive the recognition it deserves.

LYDIA W. ALLISON

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[1] Eleanor Mildred Sidgwick, "In Memory of Sir William Fletcher Barrett, F.R.S., *Proc.* S.P.R., Vol. XXXV, 1926, p. 414.

HISTORY OF THE SOCIETY

The First American Society for Psychical Research was formed in 1885, in consequence of a visit by Sir W. F. Barrett to this country, and Prof. Simon Newcomb became its President. In 1887 the Society invited a man of signal ability, Richard Hodgson, A.M., LL.D., sometime Lecturer in the University of Cambridge, to become its Executive Secretary, and he accepted.

This organization later became a branch of the English Society under the very able guidance of Dr. Hodgson until his death in 1905. The American Society for Psychical Research was then re-established with James H. Hyslop, Ph.D., formerly Professor of Logic and Ethics in Columbia University, as its Secretary and Director.

THE ENDOWMENT

The American Society for Psychical Research, Inc., was originally incorporated under the Laws of New York in 1904 under the name of American Institute for Scientific Research, for the purpose of carrying on and endowing investigation in the fields of psychical research and psychotherapeutics. It is supported by contributions from its members and a small endowment fund. The income of the Society pays only for the publications and office expenses, but does not enable the Society to carry on its scientific investigations. A much greater fund is required before this work can be carried forward with the initiative and energy which its importance deserves.

The endowment funds are dedicated strictly to the uses set forth in the deed of gift and are under control of the Board of Trustees, the character and qualifications of whom are safeguarded, as with other scientific institutions.

Moneys and property dedicated by will or gift to the purposes of the American Society for Psychical Research, Inc., whether to the uses of psychical research or psychotherapeutics, are earnestly solicited. The form which such dedication should take when made by will is indicated in the following:

"I give, devise and bequeath to the American Society for Psychical Research, Inc., a corporation organized under the laws of the State of New York, the sum of ..... dollars (or if the bequest is real estate, or other specific items of property, these should be sufficiently described for identification), in trust for the corporate purposes of said Society."